

Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux. 2^e série, tome iv., 1^{me} cahier. (Paris: Gauthier-Villars, 1880.)

THIS number contains Conférences de Géométrie supérieure by M. Saltel, in which is given an exposition of the method of analytical correspondence with two applications, the object of the one being to find the number of common solutions in k equations between k unknowns, and of the other to find the degree of a geometrical locus defined by certain algebraic conditions. The methods employed are based on that of M. Chasles's "Principe de Correspondance." The next paper, by M. Imchenetsky, "Détermination en fonction des coordonnées de la force qui fait mouvoir un point matériel sur une section conique," is an interesting one, and is founded upon a remark of M. Bertrand's ("Sur la possibilité de déduire d'une seule des lois de Kepler, le principe de l'attraction, *Comptes rendus*, April 2, 1877), "il serait intéressant de résoudre la question suivante. En sachant que les planètes décrivent des sections coniques, et sans rien supposer de plus, trouver l'expression des composantes de la force qui les sollicite en fonction des coordonnées de son point d'application." The author arrives at his result by taking his equation in the form—

$$p^2x^2 + q^2y^2 + 2rxy = (ax + by + c)^2.$$

Prof. Teixeira of Coimbra has a short note "Sur les principes du calcul infinitésimal," which calls for no special comment. Dr. G. Sous follows with what appears to us a good article entitled "Phakomètre et Optomètre." For the uninitiated "Les phakomètres sont des instruments destinés à mesurer la distance focale d'une lentille quelconque." The principle of construction of Silbermann's and of Snellen's is, when an object is placed at twice the focal distance from a converging lens, the real image of the same size as the object is situated also at double the focal distance from the lens. The objection to Silbermann's appears to be its length, which renders it awkward to carry, and to Snellen's that it is not applicable to diverging lenses.

Dr. Sous gives a form which is not liable to either of these defects, and the construction of which is based upon a physical theory, not hitherto, he states, applied to these instruments; but we must refer those interested in optics to the paper itself (fourteen pages in length). The rest of the book is devoted to "Morphologie de la membrane de Schrapnell," Dr. Coyne; "Études d'Optique Physiologique; Influence du Diamètre de la Pupille et des Cercles de Diffusion sur l'acuité visuelle," Dr. Badal; "Les Températures de la Mer dans l'estuaire Gironde et à Arcachon en décembre, 1879, et janvier, 1880," M. Hautreux; "Des Os et de leur Emploi dans la Fabrication du noir Animal, du Suif, du Sulfate d'ammonique, des Boutons," &c., M. Huyard.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Aberration of Instinct

CASES of individual variations of instinct are of importance in relation to Mr. Darwin's theory of the development of instincts by natural selection. Under the belief that aberration of instinct may be regarded as a case, more or less extreme, of variation, I think that the following instance is worth publishing in NATURE. It has been communicated to me by a correspondent on whose trustworthiness I have reason to rely:—

"A white fantail pigeon lived with his family in a pigeon-house in our stable-yard. He and his wife had been brought

originally from Sussex, and had lived, respected and admired, to see their children of the third generation, when he suddenly became the victim of the infatuation I am about to describe. . . .

"No eccentricity whatever was remarked in his conduct until one day I chanced to pick up somewhere in the garden a ginger-beer bottle of the ordinary brown stone description. I flung it into the yard, where it fell immediately below the pigeon house. That instant down flew paterfamilias, and to my no small astonishment commenced a series of genuflexions, evidently doing homage to the bottle. He strutted round and round it, bowing and scraping and cooing and performing the most ludicrous antics I ever beheld on the part of an enamoured pigeon. . . . Nor did he cease these performances until we removed the bottle; what proved that this singular aberration of instinct had become a fixed delusion was this, whenever the bottle was thrown or placed in the yard—no matter whether it lay horizontally or was placed upright—the same ridiculous scene was enacted; at that moment the pigeon came flying down with quite as great alacrity as when his peas were thrown out for his dinner, to continue his antics as long as the bottle remained there. Sometimes this would go on for hours, the other members of his family treating his movements with the most contemptuous indifference, and taking no notice whatever of the bottle. At last it became the regular amusement with which we entertained our visitors, to see this erratic pigeon making love to the interesting object of his affections, and it was an entertainment which never failed, throughout that summer at least. Before next summer came round he was no more."

GEORGE J. ROMANES

Prehistoric Europe

A few last words with Prof. Dawkins, and I have done:—

1. Having discovered that a certain absurd opinion which he attributed to me is nowhere to be met with in the volume he was supposed to be criticising, Mr. Dawkins now imagines that he has found grounds for his assertion in my "Great Ice Age," written and published some years ago. Here again he is quite mistaken. The passage cited by him, even if it be considered apart from its context, will not bear the interpretation he puts upon it. Had he read the page he quotes from with intelligent attention he would have seen that I was referring to the well-known fact that the ossiferous and Palæolithic gravels of East Anglia are represented in the North by the equivalent ossiferous *Cyrena*-beds near Hull, which dovetail with and are overlapped by glacial deposits. In other words, they rest upon a *lower*, and are covered by an *upper* boulder-clay. But I have nowhere said, nor would any candid reader infer from what I have written, that this upper boulder-clay (that of Hessele) ever extended south so as to cover the Palæolithic gravels throughout East Anglia. I am surprised that a professor of geology does not apparently understand the meaning of the term "overlap." Were I to state that in certain districts in Scotland the Carboniferous strata are overlapped by a conformable series of Red Sandstones, should I be understood to imply that these Red Sandstones formerly covered the entire area now occupied by the Carboniferous rocks of Great Britain?

2. Mr. Dawkins has accused me of having suppressed evidence which told against my views, and he now repeats this offensive accusation, citing in justification my description of the Victoria Cave, from which, he says, I have omitted all reference to the discovery of reindeer in the lower cave-earth. Now it is not true that I have ignored this alleged discovery, for I remark that "it seems doubtful whether the remains of that animal, said to have been obtained from the lower earth, really belonged to that deposit." My reasons for this doubt (which I share with other geologists) I did not consider it necessary to give, but they are simply these:—

(1) The explorations in the cave were carried on at first, under Mr. Dawkins's superintendence, by means of shaft-digging, a very unsatisfactory system of "cave-hunting," and one which, even with the most conscientious care, is liable to give false results.

(2) During the subsequent prolonged and scientifically-conducted explorations no recognisable reindeer remains were ever obtained in the lower stratum. These facts alone are sufficient to justify my scepticism. I quite agree with Mr. Dawkins, however, that the mere occurrence or non-occurrence in this particular cave of reindeer associated with hippopotamus is not of paramount importance. Even the most inattentive reader of "Prehistoric Europe" can hardly miss the statement, again and again repeated, that the southern and northern forms are often

enough commingled in one and the same accumulation. It is to account for this remarkable commingling that a large portion of my book was written.

3. Mr. Dawkins seems to be ignorant of the fact that the ossiferous deposits of Mont Perrier occur on two separate and distinct horizons. The lower bed, characterised by the presence of *Mastodon arvernensis* and other extinct forms, is unquestionably true Pliocene. It is overlaid by the "pumiceous conglomerate," with its far-transported and glacially-striated erratics. Upon the denuded surface of this well-marked morainic accumulation rests the upper bed, which contains a very different mammalian fauna—*Elephas meridionalis*, *Rhinoceros leptorhinus* (Cuv.), hippopotamus, tapir, horse, cave-bear, hyæna, hedgehog, &c. The flora associated with this fauna is not Pliocene but Pleistocene. The upper bed is overlaid in turn by a newer set of glacial moraines and erratics. The list of Upper Pliocene Mammalia from Mont Perrier and Issoire, given by Mr. Dawkins in his "Early Man in Britain," consists of a "hash-up" of the species derived from those two separate and distinct horizons.

4. The most recent list of mammalia from the lignite-beds of Leffe and Borlezza is quoted by me from Prof. Stoppani, on the authority of Dr. Forsyth Major. All the species in that list, without exception, have frequently occurred in Pleistocene beds, the age of which is generally admitted. The plants and shells associated with these species are all likewise Pleistocene forms. Moreover, as Stoppani has demonstrated, and as I can testify, the stratigraphical evidence proves that the beds pertain to the Glacial series. Prof. Mayer, no mean authority, has shown that the upper beds of the so-called Pliocene of the Val d'Arno (containing *Elephas meridionalis* and hippopotamus) are not the equivalents of the marine Pliocene, as has hitherto been the belief of paleontologists, but must be classified as Quaternary or Pleistocene.

5. All that I say with regard to the age of the skull of Olmo occurs on p. 318 of my book, and what I say is simply this, "It pertains to Pleistocene times—to the period during which *Elephas meridionalis* belonged to the European fauna." I do not assert its Interglacial age. It may be either Preglacial (*i.e.* early Pleistocene) or Interglacial as the Leffe beds are.

I was not aware that geological classification is always based on zoology alone. I am under the impression that botanical evidence, when it can be obtained, is not despised, and that stratigraphical and other physical evidence is not usually ignored. In trying to work out the historical geology of the Pleistocene, I have considered the palæontological as fully as the physical evidence. Mr. Dawkins would have me rest contented with that of the mammalia alone, as interpreted by himself.

Perth, February 19.

JAMES GEIKIE

As my name has been imported into the controversy between Prof. Dawkins and Dr. James Geikie, will you kindly permit me to state that I am quite prepared, after re-reading the account given by Dr. Geikie of the Victoria Cave, to accept all responsibility for its correctness.

Without entering into the general question, in the particular case of the Victoria Cave the evidence for the contemporaneity in the same area of the reindeer and hippopotamus is not very cogent; a review of all the evidence from that source indeed points the other way. The specimen mentioned by Prof. Dawkins was, according to his Report,¹ found in digging a shaft, a method of exploration unfortunately at that time (1872) employed by the Committee. The subsequent explorations, which were not conducted in this manner, but by carefully removing the deposits, layer by layer, to prevent any possibility of accidental mixture of the remains, gave abundant evidence of reindeer in the upper beds, but not any satisfactory evidence of its presence in the lower beds, containing *Hippopotamus*, *Elephas antiquus*, *Rhinoceros leptorhinus*, &c. This is a point, amongst others, to which, as Reporter to the Committee, I paid careful attention, and the details were impartially given in the Reports.² The absence of reindeer from a lower bed, the only one containing the same fauna in the Creswell caves explored by the Rev. M. Mello and Prof. Dawkins, is worthy of note as bearing on the same subject.

As regards the evidence for the antiquity of man from the Victoria Cave, Dr. Geikie has fairly stated both sides of the question, and he certainly does not deserve the accusation that

he "has only called those witnesses which count on his side." Prof. Dawkins, in dismissing the whole of this evidence as "founded on a mistake," must be aware that he is using a convenient formula which can only apply fairly to a part of it, the doubtfulness of which has already been fully conceded. He entirely shelves other evidences which are the result of a long and careful exploration.¹

To state that he doubts their cogency would be to take a course of which no one would complain; but to say as if it were a matter of general agreement that they are "founded on a mistake," looks like an attempt to stifle discussion.

But his remarks are so obviously polemical that to most geologists they will probably carry more amusement and less conviction than the writer contemplated.

Hastings, February 19

R. H. TIDDEMAN

Les lettres d'Outre-mer

IN the Notes, published in NATURE of January 13, p. 254, the last paragraph gives, as a fact, an announcement of "the simplest post-office in the world" in Magellan Straits, as still in existence.

At least fourteen years ago there was published a graphic account of this unique establishment by the most eminent of all living French writers, M. Victor Hugo, who introduces the circumstance into his famous work of fiction, "Les Travailleurs de la Mer"; and ever since reading the account I have wondered where the great author obtained his circumstantial relation, which refers to the year 1823. Nor can I believe that such a system of oceanic exchange ever really was in existence, at least on the spot indicated, for a very good reason; that at the point indicated, viz. the neighbourhood of Port Famine, when the *Beagle* was there in 1834 (see Darwin's "Naturalist's Voyage," chap. xi.), "the Fuegians twice came and plagued" the crew; so that an open barrel would hardly be safe. Darwin, also, who ascended Mount Tarn, the most elevated point in this district, would surely have mentioned this famous barrel post-office, had it existed (?).

I am therefore curious to know whence the note in NATURE was compiled, but I fancy the account is apocryphal. That there were however other oceanic post-offices somewhat similar in principle is a fact in reality.

In 1673 Ascension was visited by the Dominican, Father Navarette, who speaks of it then as the "Sailor's Post-Office." "Mariners of all nations being accustomed at that time to leave letters here, sealed up in a bottle, in a certain known cranny of some rock, to be taken away by the first ship which passed in an opposite direction" (Mrs. Gill's "Six Months in Ascension," p. 61). And again in 1769 we find the following extract:—

"1769, Feby. 3-4

" Ascension island.
Bougainville.

Louis de Bougainville, Colonel of Foot and Commodore of the Expedition in the Frigate *La Boussole*.

Arrd. and anchored in the North-west creek or 'Creek of the Mountain of the Cross.'

Anchorage according to Abbé la Caille.

7° 54' s.—16° 19' west, of Paris.

Variation 9° 45' NW.

Three creeks caught turtle.

N.E. creek. N.W. creek. English creek, S.W.

"In the afternoon the bottle was brought to me which contains the paper whereon the ships of every nation generally write their name, when they touch at Ascension Island.

"This bottle is deposited in a cavity of the rocks of this bay, where it is equally sheltered from rain and the spray of the sea. In it I found written the *Swallow*, that English ship which Captain Carteret commanded, and which I was desirous of joining.² He arrived here the 31st of January, and set sail again on the 1st of February; thus we had already gained six days upon him, after leaving the Cape of Good Hope. I inscribed the *Boussole* and sent back the bottle."

At page 4 of Melliss' "Account of St. Helena (1875) is a wood-cut of the South Atlantic Post Office of 1645. Speaking of the island of St. Helena, Mr. Melliss says:—

"It became about this time—little more than a century after its discovery—a resort of Dutch and Spanish ships, as well as

¹ Report on the Victoria Cave; British Assoc. Report, 1872, Sections, p. 179.

² Victoria Cave; British Assoc. Reports, 1874-75.

¹ Victoria Cave Report, *op. cit.* 1877, pp. 218-220, and 1878; *Journ. Anthrop. Inst.* vol. vii pp. 166-173.

² *La Boussole* caught up the *Swallow*, 25th February.